SCIENCE

TWELFTH YEAR.
VOL. XXIII. No. 580.

MARCH 16, 1894.

SINGLE COPIES, TEN CENTS.
$3.50 PER YEAR, IN ADVANCE.

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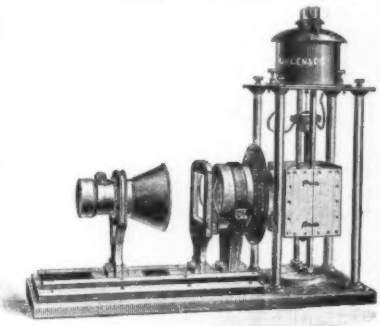

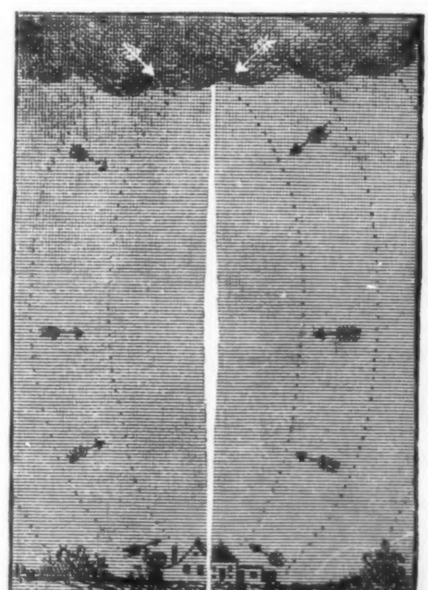

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

First inserted June 19, 1891. No response to date.

N. D. C. HODGES, 874 BROADWAY, N. Y.

SCIENCE

NEW YORK, MARCH 16, 1894.

ON THE ORIGIN OF ANCIENT QUARTZ ROCKS.

BY J. F. BLAKE, LONDON, ENGLAND.

THE abundance of large masses of tolerably pure quartz which occur in various conditions in some of the oldest formations is a remarkable circumstance which it has not yet been, I think, attempted to explain. Some of them may be put down in the first instance, though without exhausting our enquiry about them, as sandstones, as for example the "original" Huronian quartzites and the Potsdam sandstone and the quartzites of Shropshire, Sutherlandshire; others, particularly when they are gold bearing, are called reefs, which may or may not be parallel to the stratification of the surrounding rocks. These are well known to characterize the older formations in all parts of the globe. In Great Britain, as at Connemara in Ireland and Schiehallion in Scotland, there are large nearly isolated masses of white quartzite in Precambrian rocks, and, elsewhere, particularly in that portion of the series which I have called Monian, are numerous, discontinuous masses of quartz, as in the Sugar Loaf and Holyhead. There are also, in Anglesey, some still more isolated masses in the shape of quartz-knobs—small outstanding humps of white quartz rock—in the midst of a vast area of schists or ancient tuffs. It is the study of these that has suggested a possible source for a large part of the ancient quartz rocks and quartzites.

Whatever difficulties may have to be got over with regard to the crystalline or colloidal form of the substance, it is certain that from a chemical point of view the quartz of all sedimentary rocks must be derived from such original sources as produce practically pure silica. There are, I think, only three such, viz.: igneous rocks, one of whose mineral constituents is quartz, quartz veins and siliceous springs. The first of these has hitherto been almost the only source considered. To get the grains of quartz out of igneous rocks the other minerals have to be separated, and where this is done with such exceptional completeness, as in the case of a white quartzite, some unusual facilities must be supposed. Quartz veins are an obvious source of quartz pebbles, and when the latter are large, as in many Cambrian conglomerates, they seem necessarily derived from this source. As, however, quartz veins are so intimately connected with siliceous springs, both being the result of crystallization or deposit from water carrying silica in solution, they need not be considered separately.

The point, therefore, that I here suggest is that the deposits from ancient siliceous springs are an important source of the quartz of which some of the ancient quartzites and quartz rocks are composed. The starting point of this theory is undoubtedly the structure of the quartz-knobs of Anglesey. In that island there are scattered over the surface amongst the most ancient rocks, but not specially related to any particular part of them, a number of white glistening bosses of rock, which look in the distance like a whitewashed cottage only that they are usually somewhat larger. Most of these when examined microscopically show some rounded grains, and they might therefore be mistaken for ordinary quartzites. Their peculiar mode of occurrence, however, calling for more careful study, it is seen that in all there is also present a different structure, which in some belongs to the bulk of the rock or even to the whole. It is what I have called a polysonal structure. The whole area of the slide is divided up into a mesh work by clear lines, the interior of the polygons thus produced being spotted over by the minute inclusions common in quartz, which are arranged to a certain extent in relation to the edges of the polygons. (Fig. 1.) Under polarized light it is seen that each polygon is a single crystal, whose crinkly or "sutural" outlines so interlock with those of the adjacent crystals that they could only have formed in situ. (Fig. 2.) In fact, except for all the crystals being quartz, the structure resembles the granitic. Where such a structure is in small proportion to the whole it might be considered secondary, but where elsewhere the bulk of the rock is of this kind we cannot so consider it. If, however, the several crystals were to separate along these polygonal lines, they might easily be rounded into the pebble form. There is, therefore, no objection that I can see to considering the crystalline portion the primary, and the rounded grains the secondary structure. This conclusion is confirmed by certain peculiarities in the mode of occurrence. Thus in one example, which shows the polygonal structure with the greatest clearness, as we approach the knob from a distance we find the surrounding schists first veined with quartz, then the quartz veins become more abundant, then predominate over the schist, and finally at the knob itself nothing is present but the pure white quartz full of minute bubbles. Thus the knob is intimately connected with veins and has no relation to any bedded rock. In another case there is a kind of rock crossing the stratification of the schists and connected at the top with a once horizontal mass which is parallel to the stratification. These cases I take to be illustrations of the pipes of the siliceous springs.

Looking at the matter from the other side there seems no good reason to believe that such siliceous deposits as those of the Yellowstone Park and of Rotomahana should be confined to our own epoch, and yet where are

we to find these representatives in more ancient periods if not in such quartz masses as these? There is this difficulty, however, that the sample of the white terrace of Rotomahana that I have examined has no action upon polarized light and is not, therefore, crystalline. But this difficulty is easily explained by comparison with the glassy form of igneous rocks which does not prevent us looking on other rocks as igneous which are crystalline. An example of a pure quartz vein cutting like a dyke the igneous rocks of Chamwood Forest, Leicestershire, which I have examined, shows a microcrystalline structure, very like that of some felsites, especially the older ones. The association of rounded grains of quartz should also no more surprise us than the association of volcanic ashes with eruptive crystalline rocks and association so intimate in the case of some of the Italian volcanoes that it is sometimes difficult to say where one begins and the other ends; for instance, on the island of Ischia I have observed an obsidian crowded with fragments. By this comparison I would not be understood to imply that these quartz knobs are "igneous," but I think they are of hydrothermal origin, as to some extent also are true igneous rocks.

Fig. 1. Structure of a quartz knob in Anglesey seen under ordinary light.

Fig. 2. Structure of the same seen between crossed nicols.

With regard to the larger masses, if they are not of the nature of quartz veins, as many of the gold bearing reefs may be, they are probably derived by attrition, etc., from original siliceous deposits of the same nature as the above, and in this case they are not fragments of a dislocated bed of continuous sandstone, but mark the proximate sites of previously existing siliceous springs,—the sum total of the deposits from such bearing naturally no proportion to the size of the rocks up which they have been brought, but being of far greater size. Even the continuous beds of quartzite having an ordinary stratified arrangement may be derived in the first place from such sources, which will in part account for their whiteness.

There are one or two subsidiary observations which lend some support to this contention. As originally noted by Dr. R. D. Irving, the quartz grains of the Huronian quartzites are cemented by an additional growth of quartz around them, and though this quartz is of course of secondary origin, it shows that siliceous water was percolating rocks in the district. But of more importance is the occurrence of pebbles of jasper, and large ones of pure vein quartz in these Huronian quartzites. The former mineral is not an original constituent of quartz bearing plutonic rocks, but it is undoubtedly formed in the wet way. Similar jasper pebbles are formed also in Anglesey in association with limestones where these are themselves associated with the quartz rocks. There are also large tabular crystals of platz hæmatite in the white Potsdam quartzite near Philadelphia. Thus the associates of the quartz in such quartzites are not those common in granites, etc., but those which point to aqueous agencies.

Again, though more rarely, we find limestones associated with the quartzites which have the same peculiarities of distribution and mode of occurrence. These show no traces of organic structure, though there is no reason that I can see why it should not be present even in Pre-Cambrian rocks, but in some cases they do show very decided traces of a tufaceous origin. One example in Anglesey being especially remarkable, as it consists of a compound oolite, layer after layer of irregularly deposited calcite forming the coats, and a pair of the smaller bodies being sometimes surrounded by other coats embracing the two. There are also lenticular patches of bedded limestone probably like the larger quartz masses derived from these—a similar patch of tufaceous-looking, non-organic, limestone occurs in the Huronian series north of Lake Huron.

For these various reasons it seems to me that we must at least consider the possibility of the original source of the quartz in these early quartzites being the deposit from siliceous springs, possibly from the more crystalline character, bursting out under somewhat different conditions as to pressure, etc., from modern ones, and from the abundance of such quartzites we may look upon the later Pre-Cambrian period as characterized by the abundance of such springs, an idea not at all inconsistent with the supposed volcanic origin of many of the so-called Archæan rocks.

—Messrs. Frederick Warne & Co., New York, will immediately commence the publication of the "Library of Natural History," the first installments of which will compose "The Royal Natural History," to be edited by Mr. Richard Lydekker, B. A., F. G. S., F. Z. S., who is already well known to naturalists as a scientist and the author of several works on mammalia, etc. This will form, when completed, an entirely new general natural history, thoroughly abreast of the age; full, accurate and readable, anecdotic, and conveniently arranged. It will contain some 1600 illustrations and a series of 72 colored plates. This work will occupy thirty-six monthly numbers, and will be sold at fifty cents a number. Full details will be contained in an illustrated prospectus now in hand.

SCIENCE:

PUBLISHED BY N. D. C. HODGES, 874 BROADWAY, NEW YORK.

SUBSCRIPTIONS TO ANY PART OF THE WORLD, $3.50 A YEAR.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers may go directly to them. The "Exchange" column is likewise open.

THE NEW GEOLOGICAL MAP OF PENNSYLVANIA.

BY J. B. WOODWORTH, HARVARD UNIVERSITY.

THE *chef d'œuvre* of a *geological survey* is the map. It is a graphic story of the results achieved by the corps engaged in its construction, and shows by a glance wherein progress has been made in defining the limits of the natural resources of a state, in interpreting the age of its rocks, and in establishing the relations of these rocks one to another. It is a bird's-eye view of the geological history of the area, and is an indispensable adjunct of the accompanying report.

The Second Geological Survey of Pennsylvania, under the direction of the venerable geologist, Prof. J. P. Lesley, has brought its results together in the form of a final report of which the first volumes and the map have appeared. The map, dated 1893, is in four sheets on the scale of six miles to the inch, and was made by A. D. W. Smith, assistant geologist, under the direction of Professor Lesley. It is drawn on a polyconic projection, and the data were the county maps published between 1874 and 1892, together with other special maps of particular areas. The original map, so it is stated in marginal notes, was drawn on a scale of two miles to an inch and reduced by photography by Julius Bien & Co. to six miles to an inch.

The legend contains twenty-two blocks of color, two of which have overprints to indicate trap and a limestone bed, respectively. The formation column is as follows:

POST-TERTIARY.
Alluvium.
Terminal moraine.

CRETACEOUS.
Potter's and Fire Clay and Sands.

TRIASSIC (Mesozoic).
Trap, Red Shales and Sandstones.

CARBONIFEROUS.
XVII. Greene County Measures.
XVI. Washington County Group.
XV. Monongahela River Coal Measures.
XIV. Pittsburgh Measures.
XIII. Allegheny River Coal Measures.
XII. Pottsville Conglomerate.
XI. Mauch Chunk Red Shale.
X. Pocono Sandstone.

DEVONIAN.
IX. Catskill.
VIII. { Chemung. Portage. Genessee. Hamilton. Marcellus. Corniferous.
VII. { Candagalli Grit. Oriskany Sandstone.
VI. Lower Helderburg Limestone.

SILURIAN.
V. { Salina. Niagara. Clinton.
IV. { Medina. Oneida.
III. { Hudson River. Utica.
II. { Trenton Limestone. Chazy Limestone. Calciferous Sandstone.

CAMBRIAN (including lower Calciferous).
I. Quartzite.
Slate.
Phyllite.
Serpentine.
Gneiss.

LAURENTIAN.
Gneiss.

The coloration is very similar to that employed in the "Geological Hand Atlas, Report of Progress X.," which appeared in 1885, and, as in the case of that set of maps, the topographic base is omitted. While this omission leaves the colors without the variable shading of contour-lines, which is an obvious advantage where many shades of color have to be used, it detracts from the value of the map since the student cannot infer from the breadth of outcrop alone the relative thickness of a given formation. Where the outcrop is contoured, the inclination of the surface affords the trained map-reader more satisfaction.

The mother geological map of this state was published in 1858 by H. D. Rogers, from surveys made between 1836 and 1857. The eccentricity of the nomenclature applied to the rocks makes this map well-nigh unintelligible to geologists of the present day. From this first general map to the hand-atlas of 1885 was a great step, embodying the results of the first ten years of the second survey. It is interesting to note that in the present final map many minor changes in the limitation of formations have been introduced.

It is always fair to test a map by matching its boundaries with those of adjacent areas, surveyed under different auspices. The geological map of New York on the north is by no means a contemporary of the present high-grade map of Pennsylvania, and be it said, to the chagrin, not of the geologists and palæontologists of the Empire State, but of the legislators and the people, the state has no map comparable to that of the coal commonwealth. New Jersey has a geological map the matching of which with the state across the Delaware is attested by the map under discussion, on which the Palæozoic and Archæan rocks of northern New Jersey adjacent to Pennsylvania are shown in continuous bands. Of Delaware little can be said, but Maryland on the south has, through the enterprise of Professors G. H. Williams and W. B. Clark, a

geological map which, when matched along the Pennsylvania-Maryland line, brings out the single glaring defect in the beautiful chart of Pennsylvania's banded terranes. Where, between the meridians of 77°20′ W., and 77°30′ W., on the northern map the legend reads "Quartzite," the southern more accurate map shows broad fields of ancient basalts (diabase) bordered by a little Cambrian sandstone.

The three maps, of New Jersey, of Pennsylvania and of Maryland, should be in the outfit of every school where geology is taught. The student who would know how a great mountain range is constructed needs to make out a section of Pennsylvania from the ancient crystallines near South Mountain westward across the folds and faults of the Appalachians and the Alleghenies to the shores of Lake Erie. The sheets of this map brought together and mounted form an instructive wall map for class use. Both the teacher and the field student of geology must thank the author of that other memorial of Pennsylvania's history, "The Manual of Coal and its Topography," for this latest contribution to the literature of the science.

THE SYNTHETICAL POWERS OF MICRO-ORGANISMS.—II.

BY O. LOEW, UNIVERSITY OF TOKIO, JAPAN.

WE have in a former communication considered the sources of carbon for the formation of proteids, *i.e.*, for the increase of protoplasm and multiplication of microbes. The sources of nitrogen for the microbes are just as manifold. Not only salts of ammonia and nitrates, but also organic compounds of the most different structure may serve; thus: amines, amides, derivatives of urea and guanidin amidoacids and organic cyanides, *e. g.*, methylamin, acetamid, hydantoin, kreatin, glycocoll, leucin, asparagin, methylcyanide. Of inorganic combinations ferrocyanide of potassium is but a poor source of nitrogen, whilst hydroxylamin and diamid are entirely unfit for use, being very poisonous.[1] Nitrites are less favorable sources than nitrates, and the nitrates are more quickly reduced to ammonia than the somewhat poisonous nitrites.[2]

It seemed to me an interesting question how this reduction of nitrates to ammonia is carried on without the aid of nascent hydrogen; in chemistry this process could thus far not be properly explained. Evidently the living protoplasm, with its atomic motions, was engaged in this process, and I succeeded finally in applying platinum black with an aqueous solution of glucose and potassium nitrate, heating the mixture several hours upon the water-bath, in bringing about this transformation,[3] which may be expressed by the following equation:

$$\underbrace{C_6H_{12}O_6}_{\text{glucose}} + \underbrace{NO_3K}_{\substack{\text{potassium}\\ \text{nitrate}}} = \underbrace{NH_3}_{\text{ammonia}} + C_6H_7KO_8 + H_2O.$$

The peculiar kind of molecular motions in the platinum black transferred upon the molecules of sugar and nitrate brought about an exchange of hydrogen and oxygen atoms, ammonia being formed on the one hand and an organic acid (which was not closely examined) on the other. We call such processes katalytic.

The action of light is not necessary for the reduction of nitrates by bacteria or by any other plant cell. That the nitrogen is not taken as such from the nitrates for the synthesis of the proteids, but that it must be connected first with hydrogen, is shown by the nature of the ordinary proteids which contain about one-third of their nitrogen in form of amido groups.[4]

The different nitrogenous compounds in serving for assimilation of nitrogen (as methylamin, leucin, kreatin, etc.) must evidently be decomposed first with production of ammonia before the synthetical work can begin. This decomposition by the aid of the living protoplasm can take place either under reducing influences or under oxidizing ones, or by the action of water, according to the chemical nature of the nitrogenous substance; as for example:

$$\underbrace{H_2N-CH_2-COOH}_{\text{glycocoll}}+H_2=NH_3+\underbrace{CH_3-COOH}_{\text{acetic acid}}.$$

or:

$$\underbrace{CH_3-NH_2}_{\text{methylamin}}+3O=CO^2+H_2O+NH_3$$

An interesting fact in regard to the assimilation of nitrogen is the faculty of assimilating atmospheric nitrogen, of the leguminous plants, after their roots entered in symbiosis with certain kinds of bacteria, as was shown by Hellriegel. Also in this case, however, the gaseous nitrogen is not directly used for the synthesis of proteids, it must be first converted into an ammonia compound, most probably into ammonium nitrite by those bacteria.

$$N_2+2H_2O=NO_2.NH_4$$

Also this process may be imitated, as I have demonstrated, if platinum black in presence of gaseous nitrogen or air is moistened with caustic lye.[5]

In regard to the assimilation of sulfur it is also found that very different combinations can be used; thus sulfates and sulfites, methylsulfide, sulfonic acids, as, for instance, taurin, sulfones like sulfonal, etc. Evidently there must be also here formed at first a suitable group before the sulfur can enter into the forming albuminous molecule. If we consider that the sulfur can easily be split off from proteids (in part at least) in the form of sulfuretted hydrogen and that the entire character of proteids leads us to the conclusion that the sulfur is contained in them in the shape of the hydrosulfyl group, then it becomes highly probable that H_2S is the combination first formed from all the different sources of sulfur. The sulfates must be therefore *reduced* for the assimilation of sulfur. I have shown, also, here, how such a process can be performed katalytically:[6] if we heat a solution of oxymethyl sulfonate of sodium with platinum black and carbonate of sodium we can soon observe the formation of sodium sulfide. This process is certainly a very interesting sort of reduction, and may be expressed by the following equation:

$$2\,(CH_2OH-SO_3Na)+Na_2CO_3=Na_2S+SO_3Na_2+$$
$$CH_2O_2+2\,CO_2+2\,H_2O$$

Our considerations have led us to the conclusion that formic aldehyde, ammonia and sulfuretted hydrogen are the immediate material for synthesis of albuminous matter or proteids. A clue what way the synthesis may take is furnished by the decomposition of proteids taking place under certain conditions in the higher plants, whereby asparagin is formed in very large quantities. On the other hand we find that asparagin is rapidly converted into albuminous matter in presence of sulfates and glucose. Therefore the most probable conclusion is, that the asparagin is first converted into a substance, capable of yielding albumen by a so-called condensation process and that this substance must be the aldehyde of asparaginic acid. Albuminous substance would thus be formed in a nearly analogous process to the formation of sugar from formic aldehyde. I have demon-

[1] Compare O. Loew, "A Natural System of Poisonous Actions," Munich, 1893.
[2] O. Loew, Biol. Centralblatt, vol. x., p. 588.
[3] Berichte der D. Chem. Ges., vol. xxiii., p. 675.
[4] O. Loew, *Journal f. Prakt. Chem.*, vol. xxxi., p. 134.

[5] Berichte d. Deutschen Chem. Ges., vol. xxiii., p. 1443. The assertion of Schoenbein, that ammonium-nitrite is formed in small quantities, on evaporation of water in contact with air, was shown to be erroneous by A. Baumann and Neumann.
[6] Ber. d. D. Chem. Ges., vol. xxiii., p. 3125.

strated[1] eight years ago that several sugars result thus, among them a fermentable one, called by me methose and declared later to be inactive laevulose by Fischer. If we now express our view on the formation of albuminous matter in plant cells, especially in the microbes, by equations, we obtain:

$$\text{I.}\quad \underbrace{4CH_2O}_{\text{formic aldehyde}} + NH_3 = \underbrace{C_4H_7NO_2}_{\text{asparaginic aldehyde}} + 2H_2O$$

$$\text{II.}\quad 3C_4H_7NO_2 = C_{12}H_{17}N_3O_4 + 2H_2O$$

$$\text{III.}\quad 6C_{12}H_{17}N_3O_4 + 12H + H_2S = \underbrace{C_{72}H_{112}N_{18}SO_{22}}_{\text{simplest expression for albumen}} + 2H_2O.$$

This theory would doubtless indicate the simplest way possible for the formation of proteids; it is true, that some objections could be raised especially in regard to equation I., but we will at a future occasion explain what principal conclusions drawn from this hypothesis were confirmed by experiments.

[1] *Jour. f. Prakt. Chem.*, 1886, p. 321; and Ber. d. D. Chem. Ges., vol. xxii., p. 447.

INTRODUCTORY ADDRESS TO A COURSE OF LECTURES ON VULCANOLOGY IN THE R. UNIV. OF NAPLES.

BY H. J. JOHNSTON-LAVIS, M.D., M.R.C.S., F.G.S., ETC., NAPLES, ITALY.

VULCANOLOGY, or the science which concerns volcanoes and their phenomena, is a very important branch of geology, or the science which treats of the earth's crust in general. Geology is yet hardly a century old, because before that time it consisted of little else than a collection of romantic hypotheses and incredible superstitions. This remark applies with still greater force to vulcanology, for the study of which it is most necessary to possess an extensive knowledge of physics and chemistry, besides a highly developed faculty of observation. Notwithstanding, for a century or two previous to the nineteenth there were acute observers, and we in Naples well know such names as those of Sorrentino, Duca e Padre della Torre.

Towards the end of the last century the active and extinct volcanic regions of Italy attracted the attention of four great scientists, each of a different nationality: these four illustrious men were Spallanzani, Sir William Hamilton, Dolomien and Breislak. Although their nationality was different, they had two merits in common—that of scientific truth and that of Baconian methods of reasoning. In other words, they were pure scientists, since by that term we understand one who observes carefully, records neither more nor less than he observes and draws from these facts and those collected by others his conclusions without disregard to a clear knowledge of the principles involved, and without imagining facts that never existed which give rise to the enunciation of romantic hypotheses and scientific castles in the air. It is therefore more to these four men that we owe the advance of human knowledge concerning volcanoes than to all the writers who preceded them.

In the first years of the nineteenth century, vulcanological literature was enriched by many scientists, because as the allied sciences were then making great strides, they were able to offer to vulcanologists much more powerful and accurate means of investigation. Thus we had Humboldt, Scrope, Daubeny, Pilla and Gemmellaro.

Following these came a phalanx of illustrious students of that branch of geology, some still amongst us, others unfortunately dead in person but living and immortal in the memory of man as heroes of science and of human knowledge. Amongst these we may enumerate Lyell, Dana, Scacchi, Palmieri, Silvestri and Phillips, whilst at present many younger and gifted investigators are not wanting.

No other branch of science has been so heavily burdened by extravagant hypotheses which have so much retarded its progress as that of vulcanology. It is not only in the first half of the present century that we find an extensive literature, the production of men who advertised themselves as scientists when in truth they did little else but write memoirs and books to promulgate and sustain fantastic, extravagant, imaginary and impossible hypotheses. Even to-day only those who like myself have had the misfortune to be obliged to review the vulcanological literature can appreciate the large quantity of rubbish which is yearly published in the name of science. Nevertheless, amongst so much of this chaff we do meet with grain, but also very good grain.

As a subject of study, Vesuvius holds the first place in all vulcanological investigations of this and the last century. A few figures will make this fact more evident. Some four years since my wife and myself collected the titles of books, memoirs, and other writings referring to the south Italian volcanoes for the purpose of publishing a bibliographical list. We found the following numbers:

Graham's Island or Isola Ferdinandea	28
Roccamonfina	33
Lipari Islands	119
Alban Hills	210
Campi Phlegræi	539
Etna	880
Vesuvius	1552

From this table we comprehend how much has been written concerning our great active volcano, which we find constitutes nearly half of what has been written about all the volcanic regions south of Rome. If we add to these the titles referring to the Campi Phlegræi we then find that in a total of 3361 not less than 2091 concern the volcanic district around Naples. Let me, however, give you a still more striking fact. The Naples branch of the St. Alpine Club possesses the richest vulcanological library in existence. The catalogue contains more than 7000 entries of papers, books and manuscripts. However, in this number are included books that not only treat of vulcanology but in a large part refer to seismology and to a smaller extent to geology. It will be seen, therefore, that the Neapolitan volcanic district represents more than a quarter of all vulcanological literature.

It is true that the history of Etna and the Æolian Islands reaches farther back than that of Vesuvius, but on the other hand the history of this latter is by far the most complete. From a chronological point of view Vesuvius and also the Campi Phlegræi have a more important place in history than any of their rivals. Even if the Pompeiians, the Herculaneans and the Stabians did lose all their property eighteen centuries since, the modern world has recovered it as achæological treasures whose value represents, from the point of view of culture, many times the original one, and the compound interest on the same for the whole interval; and this we owe to our Vesuvius. The phlegrean region around Naples is so enchained with the poetry of the heroic and classic periods, that without it the legends of Cuma, of Pithecusa, of Sparctacus, of Partenope, of Baja and so many others, which fills pages and pages of ancient history, would not exist.

Sometimes poetic ecstasy attacks the mind of the scientist; for quite the contrary to what the general public believe, science, rather than abolish poetic senti-

ment, further developes it, but in a more serious and refined form. If you will allow me I will describe to you one of my day dreams concerning the ground that we are now standing on, and which we will entitle the "Campania Felice without its Volcanoes."

When, as we wander around Naples, we reach the hill of Cuma and we encounter a few ruined walls and a few pot-sherds that peep out through the rich vegetation of that spot, where now the only inhabitants are the goats and the lizards, our imagination speeds back for nearly three millenniums when this same rock, almost in its present state, was chosen by the daring Greek navigators as the site of their new colonial town. All of us know the history of Cuma, all of us know that this little bit of Italy for the half of historic time held a very important place. We are deeply impressed when we make an effort to conceive clearly what three thousand years really is, how many generations lived and died during that time and in that place—but far, far greater are we impressed when we think that three thousand years is but a fraction in the *geological* history of that hill, and finally our mind fails to grasp the value of time when we consider that the physical record of this hill is not more than a minute fraction of the geological records of our globe.

Without going very far back in the geological history of our region I will ask you to follow me to the first part of the pliocene epoch, an epoch, as all know, considered quite near our own time. All of us now admire the beauty of the Gulf of Naples, which has few rivals in the entire world, but at that time its conformation was very different to what it is now. It then formed a very much larger gulf, represented to-day by the plain we call the *Campania Felice* with a large part of the Terra di Lavoro. We must figure to ourselves a broad gulf, limited on the north by the promontory of Gaeta, where its confines were limited by high limestone cliffs. Its coast had roughly the following trend: From Gaeta, it corresponded with the present provincial road to close under Castelforte and from there was almost represented by the valley of the Garigliano as far as the gorge between Mt. Faito and Mt. Cammino, by which narrow strait it was in communication with the sea covering the present plain of Cassino. Winding round the south of Mt. Cammino, it again extended northwards to Mignano. The eastern coast of this strait corresponded with the present line of railway from Mignano to Taverna S. Felice, which coast turning eastwards passed under Presenzano to extend into the mountains by the valley of the Volturno. From this point the coast, winding round several islands, represented to-day by hills and mountains separated from the main mass of the Appenines, extended into these latter, forming so many fiords. The sea then covered all the plain, and its waves beat the foot of the mountains behind Pietramelara, Pignataro Maggiore, Capua, Caserta, Nola, Palma, Sarno, Angri and Castellamare and then corresponded roughly with the present coast of the peninsula of Sorrento. In the middle of this great gulf rose two important isles—Capri and Mt. Massico, besides a quantity of smaller ones. Numerous fiords penetrated the Appenines where to-day we have the Garigliano, the Volturno, Valle di Maddaloni, Valle Candina, and the Valle di Avella. In fact this part of the coast of Italy in those pliocene times was very similar in configuration to that of the Istrian coast of to-day.

The rivers brought down to the sea sand and mud which, settling at the bottom of the gulf, prepared an almost flat sea bed, which later was to form the foundation of the Campanian plain. At that period the Campania Felice was only sea, and where to-day flourish vines, oranges, lemons and gardens of flowers then only grew marine algae.

The great fissure in the earth's crust which corresponds with the western coast of Italy, and along which were formed the Italian volcanoes, opened a way for the igneous magma to the bottom of this gulf. Numerous eruptive centres were formed, giving rise to the volcanoes of Tschia, Roccamonfina, Campi Phlegræi and Vesuvius. The order in which these different groups were formed is still an unsolved enigma. Tschia, as has been long known, shows by the fossiliferous deposits clothing its flanks to have undergone great elevation since its original formation, and as we have no such evidence in the other volcanoes, we must conclude for the greater antiquity of Tschia. I also believe that the volcanic group of Roccamonfina is very much older than that of the Phlegræan Fields and Vesuvius because we find the *piperno* and the *pipernoid tuff*, very old volcanic deposits in these regions, forming a mantle over Roccamonfina when it was almost a complete mountain. It must not be forgotten, however, that in the "Museum Breccia" first described by me we have evidence of the effusion in these regions of many varieties of rocks long anterior to the *piperno*.

Gradually the large quantity of lava and fragmentary materials that were ejected at the bottom of the gulf greatly diminished its depth, and this, combined with general elevation, resulted in the emergence of a number of volcanic islands at Roccamonfina, Tschia, Naples; and probably Vesuvius was at first, like the others, an island. Constant general elevation soon drove back the sea, leaving high and dry all that region we so well know. This plain with its volcanic hills and mountains constitutes one of the most beautiful, the most fertile and the healthiest regions of our earth if man were more capable of appreciating, enjoying and developing this *pezzo di cielo caduto in terra*.

So many are the advantages that our Vesuvius offers to the student of vulcanology that I think it advisable to pass them in review. This renowned volcano occupies a very central position in the civilized part of the globe, only a few kilometres from Naples, with all the resources of a great city, and in communication by numerous lines of passenger vessels and railways with all parts of Europe and America. Means of visiting Vesuvius are numerous, whilst the volcano is now entirely surrounded by a network of railways, besides good roads. By road and railway the top of the mountain can be reached, and upon its flanks can be found hotels and accommodation of all kinds, besides a meteorological observatory intended to be used for the daily study and record of its varying phases. The simple but interesting form of the mountain, the extraordinary and unrivalled variety of its productions, which surpass in number, beauty and interest those of any other volcano yet studied, are also a matter of maximum importance to the student. Besides this, of equal importance we must reckon that continuous activity with variation within such limits as to permit detailed study on the spot and still more fully in the university laboratories or elsewhere.

Besides, scattered over Italy and within a few hours' reach are several other active volcanoes, each having its own special interest, besides a large number of extinct ones and subsidiary volcanic phenomena, all of which beyond their scientific interest have a very great importance to the inhabitants from an agricultural, industrial and hygienic point of view. This is especially the case in the immediate vicinity of the active ones, so that it becomes the duty of the government to maintain a system of observation and record and to develop a school in which students may acquire a scientific knowledge of vulcanology.

At Naples we have a chair of terrestrial physics, but as under this name is included a vast amount of different

groups of phenomena it is impossible for its holder to give a fair share to vulcanology alone. So far the only chair of vulcanology was that of Catania, which was so well occupied by the late Professor O. Silvestri and which after his premature death was abolished.

These are, therefore, the reasons why we shall give a course of lectures on vulcanology in the University of Naples. My lectures will not be simply theoretical ones, for we shall make a number of excursions into the surrounding volcanic districts, where we can examine the phenomena and materials as they occur.

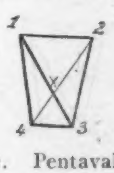
STEREOCHEMICAL THEORY.

BY T. PROCTOR HALL, PH.D., TABOR, IOWA.

In the September number of the Journal of the Chemical Society Mr. S. U. Pickering points out that the fundamental idea of the tetrahedral arrangement of atoms about a carbon atom is that of the most symmetrical arrangement of four spheres about a fifth central sphere, and that the same principle leads to a triangular arrangement about triad nitrogen and a hexagonal arrangement about pentad nitrogen. In the case of the carbon atom this idea has proved so satisfactory that we are naturally anxious to see it tested in regard to all the other atoms. It is not, indeed, to be expected that the actual relations of atoms in three-fold space can be perfectly represented by any theory which takes account only of such relations as can be expressed in a two-fold diagram. Hence, stereochemistry will inevitably become more and more prominent as our knowledge of molecules increases. It may be worth while, therefore, to consider more fully than appears to have yet been done the arrangements which the application of the same fundamental idea require for other atoms having higher atomicities. In the following list the number of possible isomers of each kind is given. I have made no attempt to compare these with the facts; indeed that may not be possible as yet in many cases; but if the fundamental stereochemical idea is to be fairly tested, one of the first necessities is that the logical conclusions from it should be fairly and fully stated, and this I have tried to do.

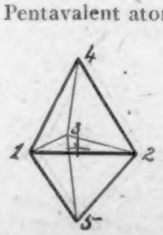
In the formulæ following M stands for the central atom, whose valance is given; A, B, C, etc., stand for monovalent elements or groups about M whose places in the diagram are indicated by the order of their positions in the formula. For example, MBA_2CB means that about M are grouped two atoms or groups of the kind A, taking the positions marked 2 and 3 in the diagram, two of the kind B in the positions 1 and 5, and one of the kind C in the position 4.

1. A Tetravalent atom may form the basis of two isomeric molecules, namely:



$$\begin{array}{c} M^{iv} \\ (2) \end{array} \left\{ \begin{array}{l} ABCD \\ ABDC. \end{array} \right.$$

2. Pentavalent atoms,—Hexahedron.



$$\begin{array}{c} M^{v} \\ (2) \end{array} \left\{ \begin{array}{l} AB_4 \\ B_4A. \end{array} \right.$$

$$\begin{array}{c} M^{v} \\ (4) \end{array} \left\{ \begin{array}{l} ABC_3 \\ AC_3B \\ C_3AB \\ BC_3A. \end{array} \right.$$

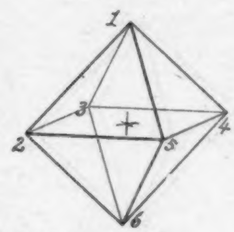
$$\begin{array}{c} M^{v} \\ (3) \end{array} \left\{ \begin{array}{l} A_2B_3 \\ AB_3A \\ B_3A_2. \end{array} \right.$$

$$\begin{array}{c} M^{v} \\ (6) \end{array} \left\{ \begin{array}{l} AB_2C_2 \\ AC_2B_2 \\ ABC_2B \\ ABCBC \\ B_2C_2A \\ C_2B_2A \end{array} \right.$$

$$\begin{array}{c} M^{v} \\ (10) \end{array} \left\{ \begin{array}{l} ABCD_2 \\ ABDCD \\ ABD_2C \\ ACDBD \\ ACD_2B \\ AD_2BC \\ BCDAD \\ BCD_2A \\ BD_2AC \\ CD_2AB. \end{array} \right.$$

$$\begin{array}{c} M^{v} \\ (20) \end{array} \left\{ \begin{array}{l} ABCDE \\ ABCED \\ ABDCE \\ ABDEC \\ ABECD \\ ABEDC \\ ACDBE \\ ACDEB \\ ACEBD \\ ACEDB \\ ADEBC \\ ADECB \\ BCDAE \\ BCDEA \\ BCEAD \\ BCEDA \\ BDEAC \\ BDECA \\ CDEAB \\ CDEBA. \end{array} \right.$$

3. Hexavalent atoms,—Oktahedron.



$$\begin{array}{c} M^{vi} \\ (2) \end{array} \left\{ \begin{array}{l} A_2B_4 \\ AB_4A. \end{array} \right.$$

$$\begin{array}{c} M^{vi} \\ (2) \end{array} \left\{ \begin{array}{l} A_3B_3 \\ A_2B_3A. \end{array} \right.$$

$$\begin{array}{c} M^{vi} \\ (2) \end{array} \left\{ \begin{array}{l} ABC_4 \\ AC_4B \end{array} \right.$$

$$\begin{array}{c} M^{vi} \\ (3) \end{array} \left\{ \begin{array}{l} AB_2C_3 \\ ABC_3B \\ ABCDC_2. \end{array} \right.$$

$$\begin{array}{c} M^{vi} \\ (6) \end{array} \left\{ \begin{array}{l} A_2B_2C_2 \\ A_2BCBC \\ A_2BC_2B \\ A_2CBCB \\ AB_2C_2A \\ ABCBCA. \end{array} \right.$$

$$\begin{array}{c} M^{v} \\ (5) \end{array} \left\{ \begin{array}{l} ABCD_3 \\ ABD_2CD \\ ABDCD_2 \\ ABD_3C \\ ACD_3B. \end{array} \right.$$

$$\begin{array}{c} M^{vi} \\ (30) \end{array} \left\{ \begin{array}{l} ABCDEF \\ ABEDCF \\ ABCEDF \\ ABDECF \\ ABDCEF \\ ABECDF \\ \text{and similarly} \\ A\text{———}E \text{ (six forms)} \\ A\text{———}D \quad \text{“} \\ A\text{———}C \quad \text{“} \\ A\text{———}B \quad \text{“} \end{array} \right.$$

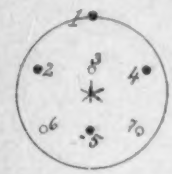
$$\begin{array}{c} M^{vi} \\ (8) \end{array} \left\{ \begin{array}{l} ABC_2D_2 \\ ABCD_2C \\ ABCDCD \\ ABD_2C_2 \\ ABDC_2D \\ ABDCDC \\ AC_2D_2B \\ ACDCDB \end{array} \right.$$

$$\begin{array}{c} M^{vi} \\ (15) \end{array} \left\{ \begin{array}{l} ABCDE_2 \\ ABEDCE \\ ABDCE_2 \\ ABECDE \\ ABCEDE \\ ABDECE \\ ABCE_2D \\ ABE_2CD \\ ABECED \\ ABDE_2C \\ ABE_2DC \\ ABEDEC \\ ACDE_2B \\ ACE_2DB \\ ACEDEB \end{array} \right.$$

4. Heptavalent atoms,—Irregular.

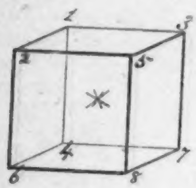
The nearest possible approach to a regular arrangement of seven atoms around one is perhaps that indicated in Figure 4, in which one atom is above M, three others are at the corners of a horizontal triangle and the

remaining three at the corners of a lower triangle alternate with the first. These positions are here assumed only for the sake of the symmetry that then appears with reference to the position 1. A very small change of positions will suffice to make the same symmetrical arrangement with reference to any of the other positions 2, 3, 4, 5, 6, 7; and it is assumed that so much motion may take place that only one form of the molecule MAB_6 is possible. Under such conditions the prediction of isomeric forms is largely guess work; but the following, which include most of the forms actually found, seem probable:



$$\begin{matrix}M^{vii}\\(2)\end{matrix}\begin{cases}A_2B_5\\AB_5A.\end{cases}\qquad\begin{matrix}M^{vii}\\(3)\end{matrix}\begin{cases}B_4A_3\\BA_3B_3\\AB_4A_2.\end{cases}$$

5. Oktavalent atoms,—Cube.



$$\begin{matrix}M^{viii}\\(3)\end{matrix}\begin{cases}A_2B_6\\AB_5AB\\AB_6A.\end{cases}$$

$$\begin{matrix}M^{viii}\\(3)\end{matrix}\begin{cases}A_3B_5\\A_2B_5A\\AB_3ABAB.\end{cases}\qquad\begin{matrix}M^{viii}\\(7)\end{matrix}\begin{cases}AB_2C_5\\ABC_2BC_3\\ABC_3BC_2\\ABC_4BC\\ABC_5B\\AC_3B_2C_2\\AC_5B_2.\end{cases}$$

$$\begin{matrix}M^{viii}\\(7)\end{matrix}\begin{cases}A_4B_4\\A_3BAB_3\\AB_3A_3B\\A_2B_2AB_2A\\A_2B_3ABA\\A_3B_4A\\A_2B_4A_2.\end{cases}\qquad\begin{matrix}M^{viii}\\(13)\end{matrix}\begin{cases}AB_3C_4\\AB_2CBC_3\\AB_2C_2BC\\AB_2C_3BC\\AB_2C_4B\\ABC_2B_2C_2\\ABC_2BCBC\\ABC_3B_2C\\AC_3B_3C\\AC_3B_2CB\\AC_2B_2C_2B\\AC_2BCBCB\\AC_2BC_2B_2.\end{cases}$$

$$\begin{matrix}M^{viii}\\(3)\end{matrix}\begin{cases}ABC_6\\AC_3BC_3\\AB_6B.\end{cases}$$

$$\begin{matrix}M^{viii}\\(22)\end{matrix}\begin{cases}A_2B_2C_4\\A_2BCBC_3\\A_2BC_2BC_2\\A_2BC_3BC\\A_2BC_4B\\A_2CB_2C_3\\A_2C_2BCBC\\A_2C_2BC_2B\\A_2C_4B_2\\ABC_2ABC_2\\AC_3ABCBB\\AC_3ACB_2\\AB_2CAC_3\\AC_3AB_2C\\AC_2BAC_2B\\ABCBAC_3\\ACB_2AC_3\\ACBCABC_2\\ABC_2BC_2A\\ABC_3BCA\\AB_2C_4A\\ABC_4BA.\end{cases}\qquad\begin{matrix}M^{viii}\\(24)\end{matrix}\begin{cases}A_2B_3C_3\\A_2B_2CBC_2\\A_2BC_3B_2\\A_2CBC_2B_2\\A_2B_2C_2BC\\A_2C_2B_2BC\\A_2BCBCBC\\A_2CBCB_2C\\A_2BC_2B_2C\\A_2CB_2CBC\\AB_3AC_3\\ACB_2ACBC\\ACB_2ABC_2\\ABCBACBC\\ABCBABC_2\\AC_2BAB_2C\\ACB_2AC_2B\\AC_2BACB_2\\ABC_2AB_2C\\AB_2CACB_2\\AB_3C_3A\\AB_2CBC_2A\\AB_2C_2BCA\\AB_2C_3BA.\end{cases}$$

The possible isomers of more complex molecules of this class are very numerous. But there is no advantage in writing them out since they are not found in practice.

When divalent or trivalent atoms or groups replace two or three monovalent atoms in the preceding formulæ, the possible forms are in all probability restricted to those in which the replaced atoms are adjacent to one another. The molecule P^vNO, for example, is of the form $M^vA_2B_1$, but only one of the three isomeric forms given in the table is in this case possible.

If there were examples of still higher valence there would be no regular arrangement possible until twelve atoms were arranged about the central atom, and these twelve would crowd one another or be necessarily so much closer to one another than to the central atom that the form would be unstable. The same cause of instability would of course prevent the formation of molecules in which more than twelve atoms are grouped about one. We have then a plausible reason why valence should not exceed the number eight.

A possible explanation of the fact that elements having odd valence remain odd and those having even valence remain even may be found in the supposition that atoms whose valence is even are symmetrical while those having odd valence are asymmetrical,—that is to say, the latter have their centre of attractive force for other atoms not coincident with their centre of gravity; and on the farther supposition that in order to form a stable compound the atoms must form a more or less symmetrical arrangement about the central atom. An atom having odd valence will then attract one other atom and form with it a symmetrical molecule. A farther addition of one atom destroys the symmetry. The addition of a third restores it. So that if symmetry is a necessary condition of stability the valence must increase by twos. On this hypothesis the stereochemical structure of molecules whose central atom has odd valence will be entirely different from what is represented in the preceding diagrams. The anomaly of chlorine, whose most stable oxygen-acid (per-chloric acid) is the one whose molecule appears most irregular in the above diagrams, disappears, and the arrangement required for the molecule of perchloric acid is perfectly regular. The arrangements for the odd valences, in fact, become the same as for the even valences of the next higher order with the omission of one point whose place is supposed to be (in part) occupied by the asymmetry of the central atom. In some cases the number of isomers possible on this theory differs from the number given in the preceding table. Such cases may decide which (if either) of the views here presented is preferable.

BOOK-REVIEWS.

Investigations on Microscopic Foams and on Protoplasm: Experiments and Observations directed towards a solution of the question of the physical conditions of the phenomena of life. By O. Bütschli, Professor of Zoology in the University of Heidelberg. Authorized Translation, by E. A. Minchin, B.A. (Oxon.), Fellow of Merton College, Oxford. London, Adam and Charles Black, 1894. 1 vol., 8vo, xvi, 379 p., with xii plates.

THIS is an attempt to determine the character of protoplasm, by analogical reasoning from the microscopical appearance and behavior, in water, of drops of oil containing soluble substances, such as salt or sugar, which, by their attraction for the water, cause it to enter into the oil and produce a solution which fills and expands the cavities previously occupied by the salt or other substance, thus converting the oil into a fine froth.

Although the author formally divides his work into only two parts, viz., I. that relating to the structure of Oil Foams and II. that relating to the structure of Protoplasm, it may be more conveniently considered as consisting of four divisions,—1. treating of the author's experiments with microscopic foams, 2. recounting his observations upon living protoplasm, 3. reviewing all known theories of protoplasmic structures, and 4. giving Professor Bütschli's own conclusions as to the physical nature of protoplasm.

In the words of the translator, "protoplasm is conceived of in this work as having the structure of a froth or foam in which minute droplets of a watery liquid take the place of air in the bubbles of an ordinary foam." The author applies to this formation a term which means pretty nearly "honeycomb structure," but which Mr. Minchin finds it more convenient to translate as *alveolus*, using as the related adjective the word *alveolar*.

The method of experimentation and comparison made use of by the author of this work is of course not wholly new, and this is not his first presentation of the subject. As far back as 1878 Professor Bütschli broached the general idea worked out in the present book, and although he has modified his views from time to time to meet the criticisms of fellow-investigators, or to accommodate them to facts brought to light by his own observations, this may be regarded as in the main a final defence of his original thesis,—namely, that protoplasm, while generally presenting an alveolar arrangement, is in itself structureless, and that its manifestations of activity are explainable on purely physical grounds.

The aims of his experiments have been, first, to produce artificial foams of the fineness of those believed to exist in protoplasm and comparable with the latter; second, to induce in his artificial plastides the phenomena of streaming, locomotion, and growth by intussusception; and, third, to show that the structure of living protoplasm and its so-called vital movements are similar in character to those produced by his methods. In pursuance of his first object he has undoubtedly found a simple method of making oil froths with a meshwork as fine as modern optical appliances can render visible, and to this extent he has matched the reticular appearances brought to light during recent years in vegetable and animal protoplasm. But, unfortunately, the microscope has not yet reached that degree of perfection, even with its latest apochromatic objectives and compensating oculars, which will enable us to speak with entire positiveness of the real form of any of the smallest structures we are able to discern, and so, with reference both to these microscopical foams and to the reticulated protoplasm of living organisms, we cannot at present depend entirely on sight and wholly dispense with imagination. In his alveolar oil-drops our author properly assumes the right to argue from the larger cavities down to the very smallest, and to assume from his employment of a structureless colloid that the boundary walls and the interconnecting threads are nothing but films of oil in different degrees of tension. But in the case of protoplasm we may fairly question whether he is thus far justified in carrying his analogy beyond the bound of mere superficial resemblance.

In pursuance of his second aim, Professor Bütschli has found that when the exchange takes place between the contents of the oil vacuoles and the surrounding water, a streaming movement is set up, the currents of which, both within and without the drop, may be made visible by mixing India ink or other coloring matter with the fluids. He has also observed that at this time the drop, which is compressed to a thin layer between glasses for microscopical examination, is likely to "creep somewhat rapidly backwards and forwards under the cover glass," a movement which, he claims, is like that of a simple amoeba. The progression is in a line with the streaming motion already referred to;—that is to say, there is a point upon the surface of the drop at which the interchange between its contents and its environing liquid is most active, towards which point the internal currents converge, and from which the external currents diverge, thus creating an anterior pole to the drop, and the author himself expresses the belief that "the creeping progressive movements are without doubt in connection with such streamings." In strongly pressed drops several centres of extension currents may arise, and, as the oil then spreads in several directions at once, there is created the appearance of "pseudopodium-like processes," but "such drops show, as was to be expected, no actual forward movement as a whole." The author remarks that "not infrequently a drop of the kind just described is observed to run towards one of the strips of cover-glass employed as supports," which plainly suggests that all of his "creeping" movements may be the result of capillary attraction, when they are not produced by the mere pressure of his compressorium.

As to "growth," Professor Bütschli is not able to present any proof beyond the simple swelling of the drops during the first imbibition of water and solution of the salt crystals entangled in the oil, by which the conversion of the oil into froth is accomplished. In fact all of the processes described are but temporary and are strictly confined to the period necessary to bring about a state of physical equilibrum in each case. Even the streaming movements are known to have continued only from twenty-four hours to a maximum of six days, apparently in proportion to the freedom allowed the drop to carry on the process of interchange, or to the quantity of matter concerned in the operation. In any case, the author admits that "the streams gradually become weaker and weaker, and finally cease," and that "the duration of the extension currents described is, for the most part, relatively short." In this respect, at least, the parallel between oil drops and protoplasm is lacking, for, according to the latest and most generally accepted belief, the one essential characteristic of protoplasm is its never ceasing activity, and it is this very attribute which now needs explanation and to which biologists are devoting the greatest attention. Professor Bütschli's experiments certainly cast no more light on this problem than do the achievements of chemists who have approached close to the synthetic construction of "the physical *basis*" without actually initiating in it the vital processes. As to the apparent similarity between the so-called creeping of his oil drops, and the purposeful and continuous migrations of the protozoa, Professor Bütschli finally admits that his explanation "seems at present feasible only for amoeboid movement in the strict sense, while other modifications of it, especially the formation of the fine pseudopodia of numerous Sarkodina, obtain no explanation." His theory of the simplest amoeboid movement is, however, itself so novel and extraordinary that we cannot but think it needs confirmation quite as much as does the phenomenon which it is intended to illustrate. Our author's belief is, that "by the bursting of some of the superficial alveoli, enchylema is poured out upon the free surface of the protoplasmic body, where it produces a local dimunition of surface tension, and in this way sets up an extension centre together with forward movement." If this theory is ever established upon a basis of actual observation it is difficult to see why the process may not be witnessed as easily in living forms as in artificial foams. On purely speculative grounds there are strong objections to the hypothesis, and some of these have made so powerful an impression upon Professor Bütschli that he is constrained at the last

moment to append a frank confession that they necessitate the admission that the explanation of amoeboid movement brought forward by him "is inadequate." Even on this point, therefore, his experiments have not furnished a tenable theory applicable to free protoplasmic forms. Add this to the author's other admission that in his oil foams "nothing was ever observed of a rotational streaming such as occurs so commonly in vegetable cells," and we are left in wonder as to what possible application his experiments can have to biological problems of any kind.

We have given so much attention to Professor Bütschli's main contention that our space does not permit of a review of his less important arguments. It must therefore suffice to say that his criticisms of the various theories of protoplasmic structure are able and interesting.

The objects of his animadversions are, however, principally those who, like Velten, Brücke and Heitzmann, have held to the necessity of an organization in protoplasm made up of more solid and more fluid parts, the more solid constituting the active reticular structure in which resides the power of contractility, the more fluid being the passive contents of the living meshwork. All that needs to be said on this point is that the theory of Heitzmann is a fair attempt to account for actually observed phenomena in natural organisms, while the speculations of Bütschli do not appear to explain satisfactorily the behavior of even his own creations.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as a proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The Editor will be glad to publish any queries consonant with the character of the journal.

Earth Worms.

THE earth worm notes and comments which have been published in recent numbers of *Science* have been of considerable interest to the writer, and this opportunity is taken to offer a few notes of observation, made at various times, bearing on the question of the cause of the appearance of large numbers of earthworms in rainy weather.

While it is not uncommon to read or hear of what are apparently well authenticated instances of "showers" of frogs, tadpoles, and fish, it is very rare to hear of any one who has seen earth-worm showers. Yet among the unobservant, the commonly received explanation of the occurrence of these animals in the large numbers that appear on our city and village sidewalks and pavements, during rain storms, is that they "rain down."

The habitat of the aquatic and amphibious animals makes it possible to accept as true the accounts of falls of such forms, because they might be taken up into the air with the water in which they live by some sudden strong uprush of air, as a tornado, but it is difficult to imagine conditions under which the burrowing earthworm could be raised to a position from which it could "rain down."

The worms which appear during rain can be satisfactorily accounted for, if a reason sufficient to bring them out of the ground can be found. Each square foot of loamy soil has, among other inhabitants, one or more earthworms living in it, so that the grass borders of streets and walks, even in large cities, harbor myriads of these animals and the soft earth in less thickly settled communities hides numbers of them beneath its surface.

Granted, then, the presence of the worms near at hand to sidewalks and pavements, cause adequate to bring them out of the earth during the rain and to make them wander about must be found. An explanation readily suggests itself, from the fact that the burrows of the animals must be full of water when it is raining, and it would seem that they would have to come to the surface or drown, and for a long time the writer was satisfied that this theory explained the appearance of the worms.

The following facts, noted at different times, tend to show that the explanation is not sufficient:

A number of years ago, while preparing some earthworms for use in a zoölogy class, I was washing them in a tank of running water, the source of supply being a small faucet tapping the village mains, the ultimate source being a small river. In the tank, which was of galvanized iron, were several crayfish, a few specimens of Onodonta and other mussels, and some snails. During the washing process, several of the earthworms slipped away into the tank, and they were left there to serve for food for the crayfish. There was no sand or earth in the tank, except a small quantity of sediment which had accumulated from the water and its inhabitants, and this was not over an eighth of an inch deep in the deepest place. Within a short time, all of the animals, except one very large specimen of a species of unio, were taken from the tank, and wishing to keep the unio alive, the water was not shut off, but left running in a small stream through the winter and the following summer. The next fall, having to use the tank for another purpose, the unio was removed and the tank cleaned. When the water was allowed to run out, in the bottom of the tank was found a large and active earthworm. The room was a private one, of which I carried the only key, and there had been no earthworms in it since the previous year, and the opening through which the water entered the tank was only sufficient to admit a slender stream of water, just sufficient to keep up circulation in the tank. The worm was carefully examined to make sure ot the identity of the species, and it was permitted to escape. The sediment in the bottom of the tank was largely vegetable in its origin, and was of such character as to furnish abundant food for an earthworm, but was even at the end of the year hardly as deep as the worm was thick. The tank was about a foot deep, and the worm had lived about a year in that depth of water.

A second case came to my notice while collecting crayfish in a small river in this vicinity. The water in the part of the stream where the collecting was done was a little less than knee deep and the stream about forty feet wide. The crayfish hide under water-logged slabs and pieces of bark from the mills above, and to catch them the wood has to be moved. Under a slab in the middle of the stream was found a live and active earthworm, which was not buried in the mud, but lying immediately on the surface of it under the slab. There had been no rain for several days, and it was not probable that the worm had been washed into the stream from the bank. These instances, together with the fact that these animals are frequently abundant in soil that is saturated with water, and the observations and records of similar and more numerous cases of the same sort, noted by Darwin in his work on the "Formation of Vegetable Mould," tend to make it plain that the earthworm is not driven out of its burrow because it fears water.

From these considerations it is probable that still other causes to explain the phenomenon must be sought, but it is not my purpose to offer any theory in explanation. The following facts, however, may suggest a line of approach, along which investigation may be made by those disposed to attempt to work out the problem. If a light tapping be made on the surface of ground inhabited by earthworms, they will come to the top of their burrows, and if the tapping is kept up they will finally crawl entirely out of the ground. The birds are well aware of this fact, and robins, in particular, make use of their knowledge of

it to get many a good meal, striking their beaks against the ground, until a worm shows its head, and then siezing it and drawing it forth. It is also said that the grotesque dances which some wading birds indulge in are solely for the purpose of attracting earthworms to the surface of the ground.

My own attention was attracted to the habit by noticing that a number of worms were wriggling about my feet as I stood talking with a neighbor in his freshly plowed garden. I had been moving about and tapping the soft loam with one foot, and the worms had appeared to find out the cause of the disturbance.

The ability of these animals, in the direction of climbing, is remarkable, and probably explains their occurrence in apparently inaccessible places, such as eave-troughs, etc., although it is not impossible that they are sometimes carried to such places by birds, or even that their eggs are blown to them by the wind and afterwards hatched.

I have seen them climb out of a Mason fruit jar of the quart size, in which there was not over an inch of earth, ascending the reverse curve at the top with as much ease as they did the straight part. In this case they were assisted by a certain amount of moisture on the inside of the jar. The conclusions deducible from the foregoing are:

First. That the worms do not rain down, but come from unpaved ground, near the walks and pavements on which they are found.

Second, That in some cases, at least, they can live for a long time entirely under water.

Third, That they may be attracted to the surface by tapping or striking on the ground.

Fourth, That they climb up perpendicular surfaces easily, even those of glass, if they are moist.

CHARLES A. DAVIS.

Alma, Mich.

Cats Hunting Snakes.

IN a late number of *Science* Mr. D. S. Martin asks for information in regard to the snake hunting habit of cats. It is such a common thing for cats to hunt snakes in this region of country that it seems to be expected of every ranch cat that she, or he, will hunt them. I have often seen my cat bring in snakes from three to four feet long. These are generally what are known as gopher or chicken snakes.

In Lafcadio Hearn's wonderfully magnificent word picture of Martinique ("A Midsummer Trip to the West Indies") he describes the grand forests of tropical vegetation in words that seem to bring them before one and then adds: "The lord of all these is the terrible fer-de-lance, the trigonocephalus, the bothrops lanceolatus, the craspodecephalus, deadliest of all occidental thanatophidia." His description of this snake is fine, and the manner in which it reigns supreme over the mountains, ravines, and forests during the day and the parks, highways and places of public resort at night shows plainly that he is right when he says the king of the island is this terrible snake. But even the king has his conqueror, and though it may be a long quotation I think the readers of *Science* will thank me for giving the words of this great master of language.

"The creature who fears the monster least is the brave cat. Seeing a snake, she at once carries her kittens to a place of safety, then boldly advances to the encounter. She will walk to the very limit of the serpent's striking range, and begin to feint, teasing him, startling him, trying to draw his blow. How the emerald and topazine eyes glow then!—they are flames. A moment more, and the triangular head, hissing from the coil, flashes swift as if moved by wings. But swifter still the strong stroke of the armed paw that smites the horror aside, flinging it, mangled and gasping, in the dust. Nevertheless, pussy does not yet dare to spring; the enemy, still active, has almost instantly reformed his coil; but she is again in front of him, watching—vertical pupil against vertical pupil. Again the lashing stroke; again the beautiful countering; the living death is hurled aside, the scaled skin is deeply torn, one eye socket has ceased to flame. Once more the stroke of the serpent; once more the light, quick, cutting blow. But now the trigonocephalus is blind, is stupefied; before he can attempt to coil, pussy has leaped upon him, nailing the horrible flat head fast to the ground with her two sinewy paws. Now let him lash, writhe, twine, strive to strangle her! — in vain! he will never lift his head: an instant more and he lies still; the fine white teeth of the cat have severed the vertebræ just behind the triangular skull."

He does not say the cats eat them. Probably they do. With us they hunt, kill and eat common snakes. A writer in the Americus (Ga.) *Republican* in March, 188c, tells of a fight between a cat and a rattlesnake, but, though the cat sought the encounter, both animals were killed.

F. A. HASSLER, M.D., PH.D.

Santa Ana, Calif.

Mesabi Iron Range.

IN my paper on the "Mesabi Iron Range," published in *Science*, Feb. 9, I should have given credit to Horace V. Winchell for the rock series, instead of to Prof. N. H. Winchell.

E. P. JENNINGS.

Ironwood, Mich.

Temperature in High and Low Areas.

IN *Science* for April 14, 1893, and again for Sept. 22, I took issue with Dr. Hann, of Vienna, on a single point in his latest discussion of this question. In the *Meteorologische Zeitschrift* for December Dr. Hann again attempts to answer my argument. The original investigation was of 27 maxima and 27 minima of pressure that crossed the Alps from Oct., 1886, to Dec., 1890. In this study, the temperature and pressure at Sonnblick (10,170 ft.) were compared with the same conditions at Ischl (1530 ft.) at the base. This would give an air column of 8640 ft. Dr. Hann found that during the passage of high areas the temperature at Ischl was *higher* than in low areas, and I took the ground that this was directly contrary to the usual, well ascertained law, and hence that this whole exhaustive investigation attempting to prove that in high areas at Sonnblick the temperature is higher than in low areas must be discarded as erroneous.

Dr. Hann now makes no attempt to explain how he obtained such a peculiar result, but claims, first, that my point is a trivial one—"Die von Herrn Hazen citirten Ziffern enthalten nur die triviale Wahrheit, dass es zuweilen bei hohem Barometerstand auch im Winter wärmer sein kann, als bei niedrigem Barometerstand." Second, he shows that in the latter part of his original investigation he proved that the usual law holds in the Alps.

I desire to note one or two points in closing my share in this discussion.

First, I protest against the use of the expression "Barometerstand" in such studies. I called attention to this in 1887 in my first article on this most important theory Dr. Hann had accepted from an investigation of M. Decherows, namely, that at some height in the air the temperature was higher in a high area than in a low area. "Barometerstand" means barometer position or

reading and may have no connection whatever with a high area. There seems to be the utmost confusion in Dr. Hann's writings in which he uses barometer maxima and minima or the above indiscriminately. It is very certain that the whole meteorologic world has understood definite high and low areas, ordinarily called anticyclones and cyclones, in all these expressions.

Second, the point I made is by no means a trivial one, as the following figures from Dr. Hann show. I will take the two colder months, Feb. and March, from his table.

Temperature Fahr. at base of Sonnblick during high and low areas:

	HIGH AREA.	LOW AREA.
Feb.	33.8°	23.4°.
March	39.2°	22.8°.

I submit that temperatures 10.4° and 16.4° *higher* in a high area (anticyclone) than in a low area (cyclone) are not trivial.

Third, Dr. Hann himself shows that the usual law holds in the Alps, for in the latter part of this same paper there is a table giving the temperature in high areas 16°.5 F. and in low areas 35°.4, or a difference of 18°.9 in exactly the opposite direction from that previously demonstrated.

I am inclined to think that these serious contradictions throw a cloud over this investigation, and it is of the utmost consequence that this be explained but if it is not, then the original contention, that temperature in the Alps is higher in high areas than in low areas, must be abandoned.

H. A. HAZEN.

Washington, D. C.

Meandering Rivers in Missouri.

PROF. WM. B. DAVIS's letter, in *Science* of November 19, contains much that it suggestive relating to the extent and phases of past denudations over the area of the Ozark uplift. In my letter of July 21, however, to which his is a reply, it was not so much my object to attempt to fix the age of the Osage River, or to define the changes of level that have taken place, as it was to raise the question whether a past base-levelling was necessary to explain the meander phenomena of this and the other rivers referred to. I there undertook to explain how the sinuosities of such streams might develop in a country which was not base-levelled. Mr. Davis, with characteristic candor, accepts this as an "important correction" to his explanation. Briefly, and expressed in general terms, the view advanced was: that, under certain conditions of declivity and stratigraphy, streams will acquire trenched meandering courses irrespective of whether the country be a flat plain or not, and irrespective of whether the lines of flow at the beginning of these conditions were decidedly sinuous or only gently curving. In any case, the radius of *developed* meanders will, of course, be proportional to the volume of the river.

This conclusion seems to follow logically from the premises that all rivers exert a sapping as well as a corrading action; or, in other words, that they tend to erode laterally as well as vertically. To produce these special results it is necessary that the declivity be not so great that lateral wear become altogether insignificant as compared with vertical wear; or that stratigraphic conditions be not such as to entirely thwart these tendencies

of running water. In a strongly flexed region, for instance, the drainage is largely controlled by the attitude of the rocks. A country of horizontal strata of moderate resistance, such as those of the Ozark plateau, is particularly favorable to the development of a swinging course. Where soft and hard beds, like shales or limestones and cherts, alternate, we can readily conceive how a stream of comparatively rapid fall may move or expand its meanders considerably while cutting only a slight depth through underlying resistant beds. Did time and space permit it would be interesting to elaborate further and to trace the effects of other modifying conditions. Without being prepared at present to express final conclusions, it seems to me probable, however, that the presence of such streams as the Osage over the Missouri-Arkansas plateau can be assigned to local conditions of declivity and stratigraphy.

Whichever hypothesis be advanced it is, of course, necessary for its acceptance that other facts of the geological history of the region be reconcilable with it. As I view the question at present, such reconciliation seems more readily effected on the hypothesis I have advanced, than on Professor Davis's. The exceptions I took to his, that the country had been base-levelled in Tertiary times, are not objections against mine. But, whether Mr. Davis be right or not as to the volume of erosion (leaving out of consideration the resultant forms) and as to the earth movements that have taken place since Paleozoic time, the explanation which I offer stands equally good.

I do not mean by this, however, to beg the questions of the extent of Mesozoic denudation and of the oscillations which have taken place since the Paleozoic period. There have undoubtedly been changes of levels; such were necessary to bring the Cretaceous and Tertiary rocks of the Mississippi embayment to their present altitudes; but I do not think the differential movements within the limits of Missouri have been very great. While the seas existed in which the post-Paleozoic deposits of Kansas and Colorado were laid down, the drainage of a part of Missouri probably flowed in that direction. With the uplift of the western area, certain readjustments of drainage must have taken place over Missouri. When I stated in my last letter that the sculpturing of the topography must have been uninterruptedly in progress from the end of the Paleozoic to the present time, I meant that Missouri had been essentially a land surface since that time. Probably the larger features of its drainage system were blocked out at the beginning of this period of emergence. This statement is not at all opposed to the idea that changes of level or readjustments of drainage took place during that period. Just what was the exact sequence of events, or the nature of the changes, I do not feel prepared to say. More critical field studies, better knowledge and more careful consideration of the geological history of surrounding areas is necessary before anything like the full story can be told. With such knowledge as we have, however, I am not inclined to accept the hypothesis of a wide base-levelling such as is required, if all of the sinuous streams of this region are assigned to that cause; and this especially when another hypothesis seems adequate to explain the phenomena in question.

ARTHUR WINSLOW.

Office State Geological Survey, Jefferson City, Mo., March 5, 1894.

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